INTERVIEWEE: C. V. L. SMITH

INTERVIEWER: R. R. MERTZ

DATE OF INTERVIEWEE: March 23, 1970

Mertz:

This is an interview with Dr. C. V. L. Smith of the Atomic Energy Commission, conducted on the twenty-third of March at the Smithsonian Institution. Dr. Smith, would you care to describe your involvement in the field of mathematics? What interested you about the subject and your early training?

Smith:

I don't know. I, more or less, drifted into it. When I was in college, I took a bachelor's degree with honors in mathematics. I also did a great deal of work in Latin and Greek, and it was a toss-up as to what I would go on in. Then I sort of slid into going to graduate school and working for a doctorate in mathematics; so, there was no very deep commitment, I believe.

Mertz:

What, in particular, in the field of mathematics at that time interested you?

Smith:

Oh, I believe the elegance of demonstration, especially in the really hard fields, like analysis, function theory and so forth, as I have always liked and still do.

Mertz:

There was no immediate, I assume, relationship between what you did in graduate work in mathematics and your later involvement in computing?

Smith:

Oh, nothing at all. Nothing. None at all. You see, during the war I was in the Navy and worked in the field of radar, so I got a good deal of interest in high speed pulse circuitry and things of that sort. And, after I got out, I went to work for Raytheon, who are from Massachusetts, working on one of the early guided missile projects. And that was where--it became quite apparent to me that really decent advances in engineering design depended very heavily on computation. We had case after case coming up where people who had applied the usual engineering way of getting to solutions fell flat on their faces.

In fact, one case we tried we did a little computation. We could have saved one of our sub-contractors from a lot of trouble but he wouldn't believe us. And, as a result, he almost tore a wind tunnel to pieces.

This was really what impressed me with the value of computation in applied science primarily.

Mertz:

Do you think it was partly because it rendered far more explicit what had previously been a somewhat more heuristic approach to problems of a quantitative nature which

had prevailed in the engineering field?

Smith:

Oh, I think so. I think it was perfectly quite apparent at that time that one of the beauties of computation was that it forced the engineer to make a reasonably decent mathematical analysis of his problems which he, presumably, would have dispensed with otherwise. And, this helped him see things that he wouldn't have seen otherwise.

Mertz:

Right. Would you care to describe the initial involvement in computing at Raytheon?

Smith:

Oh, yes. I went to a Mathematical Society meeting at Swarthmore, I believe, in the winter of 1946 or early '47 and met John Curtiss there, who was then at the Bureau of Standards. And he asked me, in a casual conversation one evening, if I thought the company would be interested in at least going into a design competition for a computer; the Bureau was in the process of procuring a machine for the Census Bureau. I said I thought we probably would, and went back to see the head of my department who, at that time, was a man by the name of Edelson, Nils Edelson. And he said, "Fine, let's go ahead." And so, we got in touch with the Bureau and entered into the design competition. I don't remember the details of it, but this was the design that really finally ended up in one of the earlier machines called, ultimately called, the RAYDAC, although it was never built for the Census Bureau. Of

course, the machine that did come out of the Census Bureau design competition was the UNIVAC. UNIVAC I; but that's another story.

Mertz:

How long were you associated with Raytheon?

Smith:

Well, I was only at Raytheon a couple of years. I helped Edelson get the group together that got Raytheon into the design. I also helped, made some contacts with the people at Sands Point, who were interested in a missile tracking and that contract ultimately ended up in the RAYDAC computer, but it was the same group that did it. After they essentially lost out to Eckert and Mauchly for the Census machine. Yes, I was only there for two years, though. Then Mina Rees talked me into coming down to Washington to take care of the computer affairs in her branch, in the Mathematics branch at ONR.

Mertz:

Now, roughly, when was that?

Smith:

Well, I came down to Washington in '48.

Mertz:

'48.

Smith:

'48. And I was concerned with computers in ONR until 1954, when I went over to the London office for nearly two years. And came back in 1956.

Mertz:

I see.

Smith:

And while I was in England, of course, I had a good deal of contact, very close contact, with the computer groups there, primarily the Manchester and Cambridge groups. When I came back in '56, I became Head of the Computing Laboratory at the Ballistics Research Laboratories and was there for three years. During that period, we entered into an agreement with the Bureau of Standards to help them finance the design studies on their Pilot machine. Our group there also undertook to build a version of this.

Mertz:

This is at ONR?

Smith:

No, no, at BRL, at Ballistics Research Labs. Because we had only the old EDVAC and ORDVAC machines there still running at the time. ENIAC we tore down the first year I was there, because it was too much of a monster to run any more.

The fellows in the engineering group up there, particularly John Gregory and Dick Bianco, did a beautiful job of adapting and, to some extent, changing the design of the

Pilot. And, actually, got a very excellent machine, which got on the air about 1960, about a year or so after they did it. Did very well, a very nice job on it. And that machine has been, for many years, the sort of mainstay of computation at BRL. In fact, Dick Bianco managed to--it was the last big vacuum tube machine built using some high-performance tubes that had been really designed for use in some missile tracking equipment.

Bianco later, however, succeeded in transistorizing the machine at a very nominal cost and did a really nice piece of work on it. It's a machine, I think, that has been curiously neglected. Apparently, I suppose, because it was one of a kind.

Mertz:

If we could go back just a little bit in your original involvement with ONR, with Mina Rees's group. When you came to Washington, what were your responsibilities?

Smith:

Oh, it was my understanding that I would be responsible for the computer affairs in the group from the beginning. But there was no formal organization as yet. However, we grew from a branch into a division about a year later, and I was formally given the title of Chief of the Computer Branch.

Mertz:

I see. And what immediately fell under your purview when you arrived?

Smith:

Well, let's see. We had--we didn't get mixed up with WHIRLWIND for a while, because WHIRLWIND was still being run out of the ONR's Special Devices Center at Port Washington and we weren't asked to take over responsibility for it for a year or so. We early got involved in the--we weren't yet involved in the Institute for Advanced Study Project, but we just about got involved in it shortly after I came.

Mertz:

This was in '48?

Smith:

Yes, I came in early '48. We got involved in the Institute for Advanced Study Project somewhere along, I guess, that year. I'm not sure. But, you see, that was primarily a matter of helping the Ordnance Corps support the project. We and the Atomic Energy Commission agreed to share costs with them, but the Ordnance Corps kept the contracting authority.

Mertz:

This was roughly about the time that ONR was being set up, if I'm not mistaken.

Smith:

Well, ONR was set up just a little bit before that. I don't know whether it was '46

Mertz:

'47, I believe.

Smith:

Or maybe '47; that was before I got there. We also had a couple of other contracts. We did have an ongoing contract with, what was then, the Engineering Research Associates, which really resulted in the first book on computers; got a group of people there headed up by Tompkins, I guess it was, Tommy Tompkins put [it] together. They also did some investigations on computer circuitry and so forth, which never really, as far as we were concerned, came to anything except just reports. That's all. Nothing was built there.

We had a small contract also out at Berkeley for a small-scale drum machine: magnetic drum machine. That was about all at the time. As I say, later, not too long after I got there, we got involved with the Ordnance Corps supporting the Institute work and then later on, about another year, we were asked to take over the administration of the WHIRLWIND Project from the Special Devices Center at Port Washington.

Mertz:

Now, in this period, if I recall correctly, this was also a time when in the early post-war years, the budgets that were being allocated for research were being rather closely scrutinized by Congress and were being cut back to some degree. Is that--

Smith:

This is true. Actually, this hurt the WHIRLWIND Project, and this was one of the troubles with the whole thing. lt had been allowed to grow in a rather grandiose way

by the people at Port Washington and enthusiastically helped, I believe, by the people who were running the project at MIT, and when we took it over, we just didn't have the kind of money that they would like to become accustomed to.

I'm not saying this in any way to cast any aspersions on the MIT group, because I think that Forrester and his associates did a very, very first-rate job on the design of the machine. I think they just--I always used to fight with them, oh, about the magnitude of the project. I always felt it probably could have been done somewhat cheaper.

Mertz:

Well, that was another question. Fairly early on, did it appear that that was a project which was costing vastly more than a number of the other ongoing projects that fell under your purview?

Smith:

Well, of course, it cost--the thing that always used to appall us was how much more it cost than the Institute for Advanced Study contracts.

On the other hand--it must be said, I think it should be admitted that with the interests in using the machine on-line--you see, the WHIRLWIND really started as a training device. The computer was to essentially provide the-- to drive a cockpit that this guy was sitting in who was learning how to fly an airplane or something of that sort. And so, they thought from the beginning in terms of on-line operation, so they had to stress, and they did stress all the way through, the questions of reliability and so forth.

Mertz:

A real time operation.

Smith:

A real time operation, which, I think, probably did mean that they had to put a great deal more engineering into it than the Institute people did. This is not to say anything against the reliability of all the circuitry of the Institute. It turned out to be pretty good. Bigelow did us a fine job and the few good engineers he had with him. They did really a first-rate job down there. The things are different in order of magnitude, different in intention. Although, offhand, the machines were much alike, as they were parallel machines; they were the two first parallel machines, and they were both designed originally to operate with electrostatic memories. WHIRLWIND did indeed succeed in having an electrostatic memory built around a special tube designed and built by Pat Youtz of the project. The Institute machine originally was planned to use this little thing called a Selectron, which was being developed in the RCA Labs at Princeton, but that never really did pan out in time. And, the decision was made to use a Williams tube memory, which worked. Selectrons finally did arrive, but many years later. In fact, I think there was one machine built with the Selectron memory.

Mertz:

Oh, yes? Is this Jan Rajchman's?

Smith:

Rajchman's Selectron. Yes. I think, as a matter of fact, Ware built a machine out at Rand, sort of an experimental machine with a Selectron memory in it.

Mertz:

Oh--

Smith:

Now whether it was Ware or--I may be wrong about that; but somebody did. I can't remember who.

Mertz:

As the ONR Mathematics Division became more formal in structure and you the head of the branch which was specifically charged with the responsibility of overseeing the Navy-sponsored research in computing, how did your branch take form in terms of its personnel and responsibilities?

Smith:

Oh, it was always rather informal. The other permanent member of it was Albert Eugene Smith, who later was in the Bureau of Ships and was retired last fall. And we had a young man by the name of Nelson Blackman for a while, who later went on to the Engineering Defense Laboratories with Sylvania out at Palo Alto, where he still is. That's about all I can say, really. We didn't have enough projects really to worry too much about structure and dividing responsibilities. As a matter of fact, it took a good deal of Gene's and my effort just to keep up with the Project WHIRLWIND. [Laugh].

It was such a big undertaking and, especially, when it started to--towards the end of our involvement--merge into, what later became, SAGE.

Mertz:

Now in the--I take it that you were periodically called upon to evaluate the status of the various research projects that fell under your purview.

Smith:

Yes. That's right.

Mertz:

Was this a quarterly or semi-annual thing?

Smith:

Well, this depended. I used to go up to WHIRLWIND quite frequently. Largely because it was our biggest project and I was--and there were always questions being asked about it. Probably went up there about once a quarter, although I can't remember.

The Institute for Advanced Study, I imagine, I--we used to go up certainly once a year and maybe twice. And, of course, I was involved there for myself from the summer of 1950 to the spring of 1951, because they asked me to come up and give them a hand with bringing the reports up to date. So I used to go there from time to time.

The other smaller things I think we took a look at once a year, where everybody was

happy.

Mertz:

Did you take on any other projects during this period that you haven't mentioned?

Smith:

No, not really. Towards the end of my time at ONR, we got involved with the University of Illinois in setting up, what they called, the Control Systems Laboratory out there. It's now called the Coordinated Systems Laboratory, so they can keep the same alphabet soup, but that was not really managed out of my branch. Although I always went to the meetings and helped write up the documents and so forth.

Mertz:

Was this the ILLIAC?

Smith:

No, no. This was a group that was gotten together about 1950, 1ate '50 or '51, and they were interested primarily in applications of computers to military situations of one sort or another. Interested, say, in computers on two or three different ships in a task force, say, being able to talk to each other and exchange information on status of forces, on tracking of aircraft primarily, things of that kind. It was an early project in that area.

Mertz:

I was wondering, in the field of ASW, it came to my attention that there were at least three different groups in the late forties that were very much interested, or at least in

varying degrees interested in the applications of computers to ASW. One was, I believe, a Canadian group, and the other was one section of the applications people in WHIRLWIND, and, perhaps, a third was up at Michigan.

Smith:

I don't know anything about those groups. I think the people that got involved in this in WHIRLWIND were probably after WHIRLWIND started to turn into what ultimately became the SAGE.

Mertz:

Ah. They were looking for other applications?

Smith:

That's right. You see, SAGE, of course, was really a pioneering effort in computers to keep track of an air situation.

Mertz:

Yeah. But in anti-submarine warfare

Smith:

You could do the same sort--

Mertz:

The problems are somewhat analogous.

Smith:

Yeah, that's right. That's right, it is.

I don't think I had any exposure to that at all, and I don't know where you would find that information. Probably you should go to some people who worked in ASW directly.

Mertz:

Then your involvement became, because of the relative financial support, more closely involved with WHIRLWIND than conceivably with others until you did go off to Princeton to participate in this report.

Could you briefly describe the stages of ONR's concern with the WHIRLWIND Project? If it can be divided that way?

Smith:

It's a little difficult to do this. I think it was--we took it over; we weren't entirely enthusiastic about taking it over from Sands Point in the first place. After a11, it was their project and, I think, change of management of that sort is never particularly satisfactory for any of the parties involved.

And I think we tended to take more of the view of the scientist who is interested in getting a computing instrument rather than the Sands Point people who always had the on-line applications in the back of their minds. Even though, I think at that time, they were a little premature.

We also were under some rather severe financial constraints which made it seem even more reasonable to concentrate on at 1east getting a decent going computer that

somebody could use.

Mertz:

Did your group have specifically in mind certain applications that you felt were more important than, say, real time applications at that point?

Smith:

Not particularly. I think we just wanted to get a high-speed computer which was well along when we took it over. Most of the fundamental circuitry had been designed. The Waylee [?] storage tube wasn't finished. A lot of the stuff hadn't been built, but the general outline was there all right. We were interested--our attitude, I think, was, "let's just get this darn thing done and get this monkey off our back and then see where we go from there."

Mertz:

In that regard, did the five-digit multiplier offer some basis for making some judgments about the

Smith:

Relia--

Mertz:

Service of the circuitry and the reliability?

Smith:

Oh, I think so. I think the fact that they built--that was a good example of the care I

think that Forrester had for reliability, was building up the five-digit multiplier and really giving it a terrific workout. I think that convinced us all that the circuitry was good and reliable. I don't think you could really come to any other conclusion after that time.

Another thing, of course, that was done at WHIRLWIND which was, I thought at the time, very good, was the very careful checking--the so-called marginal testing scheme that was built in there which allowed you to run through the machine, say, before operations every morning, and really check it out. And if there were any electronic troubles, you could locate them rather easily this way.

Mertz:

A form of preventive maintenance?

Smith:

It really was a form of preventive maintenance, that's right. But a scheme that was, I don't believe, ever applied anywhere else.

Of course, today with the electronics being fundamentally reliable, you don't have to worry about those things, really. But in the vacuum tube days, you did, and I think more people should have.

Mertz:

In that regard, what would you characterize as the important achievements, say, in retrospect of the IAS computer and of the WHIRLWIND computer?

Smith:

Well, of course, they both established the design of the parallel machine, parallel computing machine. I think the greatest triumph of WHIRLWIND really was that it really produced the first going magnetic core memory about 1953. And that was, after all, what made, really made, the modern computer a going affair.

Of course, these machines contributed very directly toward the designs. They both were studied very carefully by the IBM people, for example. And the fact that--well, Von Neumann was a consultant at IBM, as you know, and I'm sure that the Institute machine had a good deal of influence on the development of, say, the 700 series and all subsequent big machines in the IBM line.

It also was the direct parent of a number of homemade machines in universities. There were the two that were built at--two mod--versions of it--that were built at the University of Illinois. One of them for the Aberdeen Proving Ground and the other--that was called ORDVAC--and then the ILLIAC I, which was the workhorse machine for many years at Illinois.

There was really the MANIAC at Los Alamos and the versions of it, MANIAC II, and then the Chicago MANIAC III, were all lineal descendants of the Von Neumann machine.

Mertz:

In that regard, do you recall bringing in independent outside consultants to provide for you and your group assessments of the progress that was being conducted by the IAS and the WHIRLWIND machines?

Smith:

Oh, you probably mean the little committee that we had set up there at one time. Let's see, who was on it? Nyquist was on it and, let's see, someone from Stanford--you

probably have his name around there. Harry Nyquist of Bell Labs and Carl Spangenberg of Stanford University, at that time. There were also, I think, a couple of others whom I cannot recall at the

moment and we made visits, took a look at the work at the Institute and the work at MIT. These were fun, but I don't think they really did very much. I've always taken a rather dim view, I think, of traveling committees of experts who are supposed to find out all about it and run out in a day and tell you what you don't already know. They were nice guys and very able ones, but I don't think they gave us anything startling we knew.

Mertz:

Did they produce any reports as such?

Smith:

They produced a report which may--who knows--it may be tucked away in some dead files somewhere, but I bet it isn't. But, one existed. It was written up. It wasn't too long or too detailed but there was a report of this group.

Mertz:

And what, do you recall roughly, approximately what their reaction was to the work both at Princeton and at Cambridge, Massachusetts?

Smith:

Not really. No. It's so many years since I've seen the thing that I just can't remember a thing about it.

Mertz:

There were, I believe, some other outside consultants--perhaps not in the Navy--but

outsiders who did do some evaluative work--F. J. Murray at Columbia, I believe.

Smith:

Yes, Murray. Yes, I think Frank looked at some of these things because, you see, Frank was at that time--although he was a contractor of ours, with some work in numerical analysis--but he was a consultant for the Sands Point group. And, he was busily engaged in helping them with the development of aircraft simulators and so, of course, he was very much interested in the digital machines. Because Frank did an interesting thing to check out one of the big missile simulators, or aircraft simulators, that Sands Point had had developed.

He actually programmed the same thing for a digital machine and ran it on the IBM SSEC up there in New York to get a check on the operation of the analog circuitry in the, I guess it was in the Typhoon, the simulator that was developed for the Navy down at RCA Labs. So, he kept an eye on all of these developments. I don't know as he had any very particular inputs to us that influenced us in any way.

Mertz:

One thing that has come to my attention in looking into the progress of the WHIRLWIND computer, and the IAS computer, was: Apparently the late Professor Von Neumann made fairly frequent trips up to Boston to see, if not the machine itself, to talk to Robert Everett and some of the others involved there and he was, while they were in a sense rivals, he was also actually quite interested in--

Smith:

Well, I think Johnny was like that. I think Johnny wasn't proud particularly. He didn't care if any other people had some ideas. On the other hand, I think I can't quite say the same thing for the WHIRLWIND crowd. They took the typical MIT attitude that they were so much smarter than everybody else that they didn't have to talk to anybody else, and [laughing] I'm not saying this to run them down, because there are some very, very excellent people in the group, but this is typical MIT, I think. We've seen it other places.

Mertz:

One of the things that I found, it seems to be almost impossible to pin down, because there doesn't seem to be any documentation on this and, that is, that, given Von Neumann's fairly active interest in what was going on at WHIRLWIND, what, if any, ideas he might have contributed at different times? Now, I know that Everett gives him a great deal of credit for the logical design of the machine in general.

Smith:

Yeah, well, this could well be. I don't know. It seems Everett would know and I wouldn't. I didn't attend any of those. I never was up there when Von Neumann was there, so this is just outside my experience completely.

Mertz:

By the way, in passing, do you happen to recall any of the addresses or lectures that

Von Neumann gave during his career, which you might have personal knowledge of, that were recorded?

Smith:

No, I don't. I don't have any knowledge of any that were recorded.

Mertz:

Was there any, in the course of ONR's involvement in WHIRLWlND, would you say, was there any real crisis point reached in terms of the extent of the funding and the continuation of the project as such?

Smith:

Well, the funding kept going down. I don't know. It's awfully hard to answer these things after almost eighteen years. It got to the point though where Forrester felt that he had to look for other funds, and I don't blame him for this. He had his ideas of what he wanted to do and they came ultimately, I guess, out of the Air Force, because they had a--yes, they must have come from the Air Force--because they used the WHIRLWIND as the heart of a sort of experimental aircraft tracking system. It was called the Cape Cod System, wasn't it? Yes, I think it was called the Cape Code System, because there were a couple of radars stuck down on the Cape that were feeding information into Cambridge.

And that, essentially, got us off the hook, really. We were quite well pleased to see it happen. And I think they did some, apparently, some very nice pioneering work. I

used to go up and watch some of the early experiments in tracking aircraft and they got to be quite impressive.

Mertz:

One question that has come to my mind that occurred in discussing this with, oh, people like Charles Adams, who was programmer for the machine, and that is, in terms of the first actual program run: Was it a test program or a demonstration program? And it's very difficult in fact to pin this down. One thing, did this machine actually run programs for the Navy at this time?

Smith:

I just don't know. I know--you mean, before it became part of the Cape Cod System? In the time, in the few years, when it was an actually going machine? I think so. Yes, I'm sure it did. I think it did computations for other Navy contractors, things of that kind. I don't know whether anybody could re ally answer the question for you, unless there were some logbooks that still existed of machine operations. It may be.

Mertz:

Yes, I believe there are some.

Smith:

They may have been preserved.

Mertz:

But, in terms of the justification of ONR support of the project, I should imagine the

question would have come up reasonably early once the machine was running. And, that is: Well, is it indeed being used to the advantage of the Navy in terms of its application? I imagine you had to wrestle with that question.

Smith:

Ah, well, not too much. I mean people would ask questions like that, but you can always give them some sort of answer that they'll believe. And--I'm not being cynical about this. I'm just saying that the questions are usually ignorant in the first place. I just don't remember having any great violent controversies over it, that's all.

Mertz:

You touched upon one of the key problems that, I think, confronted most of the machines of its, the WHIRLWIND generation--certainly also affected the IAS computer--and that is, the extent and type of storage capability which would be either built in as internal storage or could be adjusted or adapted as external storage for the machine. It's my impression of the WHIRLWIND computer that that was a major problem.

Smith:

Well, it was. Yes, because the special purpose sort of barrier grid type storage tubes that were developed and built at MIT for the machine did not have very great storage capacity. They got so they were reasonably reliable, but they didn't have large storage capacity and this forced the group to think in terms of other means of high speed

storage. And, as you know, it was Forrester that got the idea of magnetic core and published a paper in the Journal of Applied Physics, I believe it was, back around 1950, I think. The date is well known. Where all the essential ideas, well, the works, were set forth. The only question then was to work with the core manufacturers to get cores that were of reasonable--I mean, for reasonable quality control. And, Papian, I think, did an awful lot of this --just taking cores and building them, making them himself, testing out one's manufacturers, fighting with manufacturers, and finally actually, getting a memory together that really worked; and this was done about the end of 1953 or something like that. And this, I think, was one of the great achievements of the WHIRLWIND Project.

Mertz:

Could you point to any particular achievement of the IAS computer? You touched upon its being the prototype or having stimulated the development of a number of other machines, such as the ILLIAC, the MANIAC series.

Smith:

Well, I don't think there was any single technological achievement to compare to the magnetic memory. The internal organization, though, the asynchronous machine, I think, had a great deal of influence in various designs. The circuitry was certainly very nice, but what can you say? There are so many ways of doing these things.

Mertz:

Well, it might have also been the case that because of the interest of a man like Von Neumann and the location of this activity at the Institute, [it] did have another kind of reverberating influence in the academic world in computing machines.

Smith:

Well, this is true. I think that Von Neumann and the Princeton group probably had far greater influence among the academic types than WHIRLWIND ever did, because the WHIRLWIND group were essentially engineers. They had a very definite thing in mind. They did not go out talking, say, to applied scientists and so forth all over the country the way Von Neumann did. So, I don't think they had the influence that the Von Neumann group had. That's perfectly true.

Mertz:

Well, of course, the stature of theproject, Forrester was a relatively young man,

Smith:

Oh, yes, Forrester was a young man.

Mertz:

Von Neumann was a world-renowned scientist.

Smith:

Yeah, that's quite true.

Mertz:

That might have something to do with that.

Smith:

I think it does certainly. Certainly, it had a great deal to do with it.

Mertz:

One other element that has emerged in reviewing some of the activities of WHIRLWIND is the, perhaps obvious, but yet relevant development of test equipment. When you're dealing with machines that are sort of one of a kind in this, more or less, first generation of machines, the test equipment has to be developed almost simultaneously with the actual componentry of the machines.

Smith:

Eh, that is true. The WHIRLWIND people were very careful about building test equipment. I mentioned also before that they did devise this scheme, which, I believe, was Forrester's also, the marginal checking, building a marginal checking, which is extremely effective. I don't believe--I remember at the Institute Bigelow was very careful in designing his circuitry to design for large tolerances, to guard against failure of the electronic components. However, he did not go into any really large-scale program of test equipment or things of that kind. He did not build anything like the marginal checking. The machine, though, was a reasonably reliable machine. It ran pretty well.

Mertz:

Do you recall what the relationship was between the university and the Institute in

terms of the management of this activity?

Smith:

Where?

Mertz:

At Princeton.

Smith:

Well, the university had nothing to do with it. You see, it was strictly the Institute for Advanced Study.

Mertz:

I've come across a reference that the Research Projects or Research and Engineering Department at Princeton University apparently has some--this department has some records relating to the Institute computer and how they got them is a mystery to me.

Smith:

Very curious. I don't understand that at all because the two organizations are completely separate. Very curious.

Mertz:

The actual engineering support then for the Institute computer had, more or less, to be drummed up by the people on the staff itself?

Smith:

Oh, yes. That's right. I mean, the construction of the machine was financed by--it was

carried out on a contract with Army Ordnance to which the Atomic Energy Commission and the Office of Naval Research contributed funds by transfer to Ordnance and then Ordnance transferred all the money to the Institute. And that's the way the machine was paid for.

Now, I'm not sure, later on, you know, after, well, say, in the middle fifties, after when the machine was a going affair, whether Princeton might have spent some money there to have some computation done or something like that. This is always possible. I would have just absolutely no information about this and I don't know how you'd find out. Does it say anything in these records that you've seen?

Mertz:

Well, only in reference that there apparently might be some material available in the Research and Engineering Department of the University.

Smith:

Oh, I see. You haven't seen the material?

Mertz:

I have not seen the material.

Smith:

I see. Where did you get this information? I'm curious.

Mertz:

From the Institute.

Smith:

From the Institute? Well, who knows.

Mertz:

One of the other things, it is something of an anachronism or a curiosity that an essentially theoretical Institute would undertake a problem which is (a) an applied mathematics problem and (b) has some very definite engineering problems.

Smith:

You mean like the design of the machine and the building?

Mertz:

Yes.

Smith:

Oh, well, but it was very simple. Von Neumann let it be known that he either jolly well-built the machine or he left. And they couldn't afford to let him go. [Laughter].

Mertz:

Did he view, aside from the possible use of the Selectron--and I've reviewed some of the early literature published by Rajchman on the Selectron--aside from that, do you recall whether they did use any of the engineering capabilities of the, what later became, the David Sarnoff Research Center of RCA?

Smith:

I don't really believe so. The time that I spent around there I saw--I don't think I ever

saw anybody from the RCA Labs over visiting there. I believe all the circuit design was done right internally. Oh, they probably had good friends over there. I know Julian Bigelow used to go over and give talks and things like that, but I would say that as far as digital circuitry and logic were concerned, that the Von Neumann group was really far ahead of anything that they had at the RCA Labs.

Mertz:

And they then, in turn, spread the word throughout the academic world from the Institute: through visiting fellows--

Smith:

Oh, yes, visiting fellows would drop in and see it. And, of course, Von Neumann was always going around giving lectures here and there, and I've heard him give the same lecture a number of times. It was always good, but [laugh]--

Mertz:

We would very much like to find a tape recording.

Smith:

I know. I know. It's too bad. Of course, there are always variations to it. But, yes, I think so. The word got around to a lot of people that way. Although you'd be surprised how few people it really got around to.

Mertz:

Well, in the field of mathematics I should think a number of people--

Smith:

Well, not mathematics because, after all, most mathematicians aren't interested in computation. I think it may have gotten around more to--well, I don't know. I really don't know. Probably to physicists.

Mertz:

Yes.

Smith:

And people in various fields which used to be considered physics but aren't, like fluid mechanics and things of that kind. You see, some of the early applications that they're interested in --Von Neumann was interested really in fluid mechanics, and the motion of the atmosphere, for example.

Of course, the fact that Von Neumann was a first-rate mathematician himself meant that mathematicians, however pure, really couldn't despise the project. They had to love Von Neumann and had to love his dog, too, I guess.

Mertz:

Well, it helped increase--

Smith:

And, of course, this did not have that ingredient at MIT for some reason. Forrester was Mr. WHIRLWIND and the people--the rest of the faculty didn't seem to really get very excited about it. At least, I couldn't see that they did.

Mertz:

I know that that seems to--

Smith:

They should have, of course.

[END OF SIDE ONE]

[START SIDE TWO]

Smith:

... It was like the on-line use and so forth in real time situations than he was in computation.

Mertz:

Well, and also in getting involved in those things which do require a certain sophistication mathematically, in terms of applications. There also appears to emerge--on this, I have a great deal of difficulty documenting--

Smith:

Is this on the air now?

Mertz:

Yes, we're on the air. Something of a rivalry between the older tradition of the Aiken research at Harvard, which was more table oriented, in terms of developing fundamental tables, and WHIRLWIND and, to some degree, there was, maybe a little bit, I don't know, mutual or maybe one-sided rivalry or non-involvement. It's

surprising, for example, that you find in such close proximity two groups who are working on machines for computational purposes with such little contact.

Smith:

Does Macy's tell Gimbel's?

Mertz:

[Laugh].

Smith:

Yes, well, you're quite right about this, but let's face it, Howard Aiken was an arrogant character. [Laugh]. He probably looked upon the guys down there as a bunch of pipsqueaks. I'm sure he wouldn't have condescended to have gone down and asked them any questions, which he could profitably have done. So, it's not hard to understand, ridiculous as it is.

Mertz:

And, also, perhaps, given what seems to emerge, at least from some of the documents of the time, a rather defensive stance on the part of the WHIRLWIND project itself vis-a-vis the rest of the world. They seemed to be a little bit--

Smith:

Well, they felt--look, when we couldn't find enough money to keep them in business the way they wanted to be kept, they felt sort of hurt by the whole thing, which is too bad, and I regret it, because I have many times said that I thought WHIRLWIND was a

good project. It did good work. Forrester was very good--managed it very well. Forrester, I think, is an extremely brilliant man and I think the recent work that he's apparently doing in the area of urban dynamics at the Sloan School of Management is extremely significant. They may have felt somewhat defensive. They probably thought that we were trying to put them on the defensive when we were only just trying to say, "Look, boys, we got so much dough, and this is it. The money tree ain't got any more leaves this year." And they found it, I think, a little hard to believe; but this is the situation that one frequently runs into. It's tough.

Mertz:

When it got up to the higher echelons at MIT, Stratton and others, I think there was a greater appreciation of the nature of the problem the Navy was faced with, in terms of financing.

Smith:

Well, there probably was. This is--somebody looked into this whole matter and wrote a draft, a book on the whole subject--a couple of management expert types a year or two ago, and I don't know if they ever got around to publishing it or not.

Mertz:

Kent, Redmond and Smith.

Smith:

Yeah. Did they publish it?

Mertz:

It is at MIT press. I don't know whether it's been published or not.

Smith:

I see. Well, I read over a good deal of the manuscript and, as far as I'm concerned, it was OK. They may have knocked the Navy a little bit too much in there. I'm not sure that they did.

Mertz:

Well, some of this is very hard to document. I find that the interpretations--you can apply a variety of interpretations to the evidence, at least, which they've used.

Smith:

But you always can do this in any situation. What do you know for sure? You don't. You just don't, that's all.

Mertz:

Well, sometimes one can interview some of the participants and get a feeling for what they--for example, in the case of Jay Forrester. His view today is not what his view was at the time the project was faced with a very grim financial outlook. He tends to be far more, perhaps, judicious in seeing the position of the Navy.

Smith:

Well, I think he probably does. Look, Jay, was faced with great problems and he was doing a big job. And I've got no criticism of him really.

Mertz:

But, at the time he probably felt ...

Smith:

Oh, I think he felt much more involved and I think he felt that we were trying to be mean to him, which we really weren't. And, as I say, I have great admiration for Jay.

Mertz:

They also, if you take the IAS, WHIRLWIND and the Aiken project just down the river a piece, you can also see that they were in a way the junior people in age, perhaps to some degree in experience, in what they viewed as a rather competitive situation, particularly in the case of the IAS computer, because I think that was one that very logically could be compared. They were two projects going on at the same time.

Smith:

And with much the same kind of philosophy, except that, well, they were a synchronized machine where the IAS one turned out was asynchronous. But they were parallel machines and they both demanded high speed memories to make them work decently. And the circuitry, of course, was entirely different due to the different design philosophy.

Mertz:

And also, the great stress that was placed on component reliability and testing.

Smith:

Yeah, with great intercourse that was a thing at MIT which was very important. But, when you look at the WHIRLWIND work and the Harvard work, all you've got to ask yourself is what influence in the long run did the Harvard machines have? I mean it doesn't seem to me that they developed any great technological essentially breakthrough, like the magnetic core memory, which has really made modern computation go.

Mertz:

Well, one influence you might want to consider is the possibility--now I'm looking at it from the history and technology generally--and that is, the involvement of IBM with Aiken in the project at Harvard was an obvious extension in a sense of an academic commitment on the part of a corporation to involve itself in this sort of research. And, while they perhaps jumped into the picture late, they certainly jumped into it in a very timely way with regard to high speed electronic digital machines.

Smith:

You mean IBM?

Mertz:

Yeah.

Smith:

Yeah, but I think this wasn't due to Aiken. I think this was due to John Von Neumann.

Mertz:

Oh.

Smith:

You see. When did IBM really get going in the electronics business? It's really the 701 has the--and, of course, on a lower level, the 650. But I don't think they ever would have built a 701 without John Von Neumann. That's the way I read it. I may be wrong, but I don't think they got anything out of their involvement, really, with Harvard. Sure, they helped build--well, they did actually put together the MARK I, but I didn't think that IBM was involved with the Harvard Lab when the Harvard Lab finally got to building electronic machines, MARK III. And I can't see that the MARK III really had any influence anywhere. Perfectly good machine, but it's--I don't think that they had the engineering--well, they didn't press things the way the guys down the river did. They weren't interested in one-tenth microsecond circuitry and things of that kind, which Forrester's fellows did and did it all right. They knew what they were doing.

Mertz:

The only thing that was asynchronous about their machine was the external memory, but so far as the internal memory and everything else, it was fully synchronous.

Smith:

So, yes, I got into trouble, of course. That elaborate system they had to use for DC

restoration and so forth, all this business, which you avoided, you see, and you went to direct coupled circuitry of the kind that Bigelow used. Those problems just disappeared.

Mertz:

Well, Norman Taylor apparently came with the group. I gather he played some of the role as chief engineer.

Smith:

I suppose so. I could never remember quite who was. He and Everett were pretty well up in the organization, too. They're both bright guys. Norm Taylor is a real good man, and Gus O'Brien had quite a lot to do, I guess, with input-output and things of that kind. Gus O'Brien was really good. There were a number of real good, young engineers on that project. There's no question about that. They had them. I think they had much better ones than Aiken had at Harvard.

Mertz:

And, of course, the whole scope in terms of the numbers of people involved was so much greater than at the Institute that the amount of engineering elegance might tend to be overlooked at the Institute because of the fact that there were relatively fewer people involved, and it's not looked at as an engineering product as much as the WHIRLWIND computer.

Smith:

You mean you're talking about the Harvard one?

Mertz:

No, I'm thinking now of the IAS one.

Smith:

Oh, the IAS one.

Mertz:

In comparison with the WHIRLWIND.

Smith:

Well, of course, hm, I don't know, it's hard to make these comparisons, really, when you consider the difference in motives and the difference in personalities and all that. I always thought that Julian Bigelow was much like the old-fashioned Yankee inventor, you know. He'd tinker away somewhere until he came up with something, but he didn't want a whole bunch of people into the act. And he often, I think, he came up with extremely ingenious solutions to various circuit problems and so forth. At least that is my feeling for it.

Mertz:

In the area of software, what would be called programming at that time, do you think the--can you make any general assessment of the IAS machine? And its influence, or was it not particularly?

Smith:

Gee, I don't think it's possible to make much of an assessment of the influence, because there wasn't any real software in those days. There were, you know, people who wrote machine language. There wasn't any real assembly language, really, and certainly nobody had thought--well, compilers had been thought of, but not implemented. I guess the idea of a compiler goes back to the early fifties, but that was not in any of those groups. That was really in the UNIVAC crowd up in Philadelphia, and so I don't think that there was any question really much of influence around the thing.

Let's see, it was my impression that a lot of the family, the library of sub-routines and so forth at WHIRLWIND were actually done under the direction of--wait a minute--somebody from England. What's the guy, one of Wilkes's associates, wasn't it? Didn't he spend a year or so up there?

Mertz:

Well, there's Welchman.

Smith:

No, not Welchman. I don't think so. No, it wasn't Welchman.

Mertz:

Probably William Porter. He came in later under the SAGE system.

Smith:

No, I was just trying to think. No, this was a guy who was also a fellow of St. John's College, Cambridge, and whose name I should know because he spent several years at

Illinois later on.

Mertz:

I have a very imperfect roster of personnel here.

Smith:

Oh, brother!

Mertz:

Phillips did get involved in an early stage in some help but he's not from Cambridge.

Smith:

Where is Phillips from?

Mertz:

MIT.

Smith:

He's MIT. David Brown, Norman Wilson, Norman Taylor. Oh, I remember Harris Fahnestock. I'd forgotten about him.

Mertz:

Franklin was--

Smith:

Oh, Philip Franklin was--yeah.

Mertz:

Was involved. Now it could--and Morse.

Smith:

Yeah. Here's the guy I mentioned that worked on the cause.

Mertz:

Papian. Yes, he's now in St. Louis.

Smith:

Yes, he went out there. He and Wes Clark went out there four or five years ago. Now I saw Wes Clark just lately. He's grown a beard. He looks quite nice though. It's a neat one.

Mertz:

Of course, Wieser is here in the Pentagon.

Smith:

I see, I see.

Mertz:

He's under Foster.

Smith:

I was just trying to think about this fellow, but--wait a minute, David Wheeler. Didn't David Wheeler spend some time at MIT? Didn't he help them set up some of their program library?

Mertz:

Could very well have. Well, Wilkes also spent some time.

Smith:

Yeah, but I think actually Wheeler actually helped pull the program library together.

Mertz:

Yes, and there was perhaps a more elaborate development of the program library than anything at the Institute machine.

Smith:

I think so. Yes. I think this is true. See, one thing they had a lot of bright young guys, like Charlie Adams and so forth, on there that they could put to work writing programs and things like that. And, I think, Minsky was involved in that sort of stuff, so they had a lot of real smart graduate students that were looking for something to do and they put to work.

Mertz:

One thing I was highly amused by is the fact that in his--oh, I don't have it here--but his Master's paper, Charles Adams, was writing a program of taking--it was on ship, on a naval design problem, which was never run on a machine but just simply to go through the exercise of "how do you set up a problem for WHIRLWIND?" He did this rather early, before the machine was running.

Smith:

Yeah, I know they tried out an awful lot of programming. They did a lot of programming before the machine was on the air. In fact, the guys were pretty

impatient to get on it and see whether the stuff would work or not.

Mertz:

Now I'd like very briefly to, if I may, cover your career after your return from England where you spent two years there and then went to the BRL. And how long were you associated with BRL?

Smith:

I was with BRL from the spring of '56 to the summer of '59. Yeah.

Mertz:

And then?

Smith:

And then I went with NASA out at Greenbelt, Goddard Space Flight Center, trying to get a computation--well, actually what we call a Data Systems Division. We had the prime responsibility for things like computation of orbits, and things of that kind.

Mertz:

That's quite a massive--

Smith:

It's a big operation, yes, it's a big operation. And, of course, we were in constant conflict over who got the computers, and, although we nominally had control of several of them, we also had to give them up to Project Mercury, which was in a great flap in those days trying to get their programs checked out. It was not a particularly happy

situation. I think I managed to build the department up from about 2 or 3 people to about 150 in the nearly three years I was there. We were not helped by the fact that the director at the Lab at the time wasn't very bright and could be easily led down the garden path by anybody that really would give him a good story. So that was the reason I left there.

Mertz:

That was in '62?

Smith:

I left in '62. I considered I had enough of slumming around that place and I went out to the Atomic Energy Commission that year to take up the job that John Pasteur [?] had vacated the year before and they hadn't succeeded in getting anybody to fill.

You know Pasteur [?] ?

Mertz:

No, I don't.

Smith:

Oh, I see. He's coming in. He's been down in Illinois ever since then. He's coming into Washington this spring to be head of the Office of Computer Affairs at the Science Foundation, one of the group Milt Rose headed up last year, until last summer.

Mertz:

I see. Well, I think he himself has been interested, involved in--

Smith:

Oh, Pasteur has been heavily involved. In fact, he was associated in doing some computations with Fermi and Gumb [?] back in the early fifties at Los Alamos. They did some computations out there that nobody still understands. Very fundamental ones on multi-body systems.

Mertz:

I hope, is it Metropolis?--understands some of it.

Smith:

Well, I don't know. There were some things I think, according to John, they were never able to interpret satisfactorily in some of these early multi-body calculations. Very curious but, anyway, John's a very good man, because he's both interested in applications and he's interested in computer architecture, too.

Mertz:

And you remained with the AEC since that time?

Smith:

Oh, yes, I'm too old a dog to change any more. I hope I'm going to retire before very long and get out of this foolishness.

Mertz:

And you have been primarily interested in evolving applications?

Smith:

Well, actually we have an ongoing program in, what we call, computer R&D, and we have one in applied math, and we have one in programming languages, things like that. We should really probably take two--take the language and the hardware program and put them together and call them computer science or something like that, but maybe we'll get around to it someday. Accountants don't like to change these things, so you're probably happier if you can just stay with the old terms. As long as I have no future, I don't really care.

Mertz:

Well, let me ask one question about the AEC itself, in terms of its early involvement in computers, which is clear that it was.

Smith:

Oh, very heavy, yes.

Mertz:

Do you know--this I honestly don't know--whether their early involvement is now unclassified and, if so, whether there is any hope that some of the documentation is--

Smith:

Oh, I see. Yeah. I really don't know. I couldn't answer that for you because I know, for example, that they sort of pre-empted large amounts of time on things like the Bureau of Standards machine. And later on, the UNIVAC that Polachek was

running out here at the Model Basin and things of that kind. But, whether that stuff is de-classified or not, I just wouldn't know. But, from my point of view, I think the interesting thing is that due to Von Neumann the Commission interested itself in computer development.

Mertz:

He was also, of course, among many other things, I think, a consultant?

Smith:

That's right. Before he was a Commissioner, he was a consultant and then, of course, he was a Commissioner later on. And he--

Mertz:

He was also a consultant to the Navy--

Smith:

That's right. He did consu1t with a great number of people at different times. There's no question about that. Well, you see, the Commission did help fund him, his activities at Princeton, as we were talking about before. The Commission also established, early in the game, let's say about '52 or something like that, a Computation Center at New York University, with one of the early models of the early UNIVAC Is in it. And, don't forget, also, that a number of the laboratories, several of the laboratories, developed machines essentially of the Von Neumann family.

Mertz:

Los Alamos you touched on.

Smith:

Los Alamos, also Argonne.

Mertz:

Yes, Argonne at Chicago.

Smith:

Yeah. Yes, they built a couple of machines there. Let's see, there was, the Oak Ridge machine, the Oracle. Now let's see, was that actually built at Argonne and shipped down to Oak Ridge? It might have been. I'm not sure. There was one called the AVIDAC. There was later one called the GEORGE at Argonne. These were all, more or less, versions--they had strong family resemblances.

Mertz:

Did they have--this was later perhaps out on Long Island?

Smith:

And then the machine on Long Island--what was that called? I should remember. MERLIN, I think.

Mertz:

Yes, that's right.

Smith:

MERLIN actually was still being used a couple of years ago. It was devoted entirely then to sopping up information from some online experiments, things of that kind, but was something that there is still a useful function for. MERLIN, I guess, was about the last of the projects and in lots of ways the least successful but it turned up in operable hardware.

Mertz:

Would you have any suggestion about where one could go--well, perhaps the historian's office at the AEC.

Smith:

I would think that would not be a bad idea. To get what sort of information?

Mertz:

Some of the early documentation.

Smith:

The early documentation to find out whether it's still classified and things of that kind. Yeah, I suppose that would be the sensible thing to do. Who was the historian out there? I never can remember his name.

Mertz:

Well, I think it's changed, but the AEC produced a very, and one of the more successful, institutional history projects.

Smith:

Yes, I know. I've seen that, but I don't know anybody that's involved in that.

Mertz:

He didn't go into the kind of technical detail that we would be interested in.

Smith:

Yeah, well, that's the trouble. Yeah, yeah. It's hard to get that kind of detail.

Mertz:

And that requires usually documentation, to be able to do it right.

Smith:

Yeah. And I think another thing, too, that the Commission's activities have helped encourage the computer manufacturers to develop faster and better machines for scientific calculation.

Mertz:

sYes.

[END OF SIDE TWO]